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WRITTEN TO PROVIDE INVESTIGATORS OR ADMINISTRATORS WITH A REVIEW OF THE PROBLEMS OF PLANNING, CONDUCTING, OR INTERPRETING STUDIES IN HUMAN RELATIONS TRAINING, THIS PAPER DISCUSSES RESEARCH PROBLEMS AND WAYS TO OVERCOME THEM. PROBLEMS OF SELECTION OF CONTROL GROUPS, TEMPORAL CHANGE IN TRAINING OUTCOME, DESIGN RESTRICTIONS IN OBSERVATION OF EXPERIMENTAL OUTCOME, TIMING OF DATA COLLECTION, EXPERIMENTER-PARTICIPANT RELATIONSHIPS IN LABORATORY SETTINGS, AND STATISTICAL MEASUREMENT ARE REVIEWED. AMONG THE SUGGESTIONS FOR RESEARCH IMPROVEMENT ARE--TO STUDY THE PROCESS OF TRAINING AS WELL AS THE OUTCOME (FOR MOST VOLUNTEERS DIFFER SIGNIFICANTLY FROM NON-VOLUNTEERS, THE LATTER SHOWING A HIGHER DEGREE OF RESISTANCE AND MISTRUST AFFECTING RESULTS), TO EXTEND OBSERVATION OF TRAINING OUTCOME OVER A LONGER PERIOD OF TIME (CHANGE IN BEHAVIOR CAN BE PROGRESSIVE AND SIGNIFICANT), AND TO INVOLVE THE EXPERIMENTER MORE WITH THE PARTICIPANTS (TO OVERCOME THE STERILE ATMOSPHERE OF THE TEST SITUATION WHICH AFFECTS PARTICIPANT ATTITUDE). (THE DOCUMENT INCLUDES 14 REFERENCES.) AN EARLIER VERSION APPEARED AS ED 011 369 (AC 000 653). (PT)

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EXPLORATIONS

Human Relations Training

and Research

Number One - 1967

Problems in the Design and Interpretation of Research on Human Relations Training

by

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The measurement of processes of learning and the evaluation of the outcomes of the teaching-learning process have always posed major problems in research design. This is especially true where the desired outcomes are broadly defined as changes in interpersonal behavior: A complex of knowledge, values, perceptions, and behavioral skills. Until recently, behavioral scientists have tended to avoid research on human relations training in favor of more "researchable" topics which lend themselves to rigorous experimental design.[1] In addition to or because of the difficulties in constructing respectable designs, there have been relatively few available publication outlets for such research, and this has further restricted the number of studies which have come to light.

Recently, however, there has been a substantial increase in published research on human relations training, and there appears to be increased interest among behavioral scientists in conducting studies in this area. This paper is written in part to provide these investigators with a review of the problems which should be thought through when planning and conducting such studies.

Unlike much research in the behavioral sciences which is primarily intelligible and of interest only to other researchers, research on human relations training is frequently written for and read by administrators and practitioners who may base important action decisions on the findings of the research. This latter group has an important need for guidelines which will aid them in evaluating the soundness and applicability of research reports to their practical decisions on the planning and conduct of training. This paper is equally addressed, then, to the creators and consumers of research on human relations training.

Much of the research in this area is concerned with the special variation of training known as "sensitivity training," T-Group training, or laboratory training in human relations. My own experience and familiarity with the literature are also focused on this approach, and the examples and references in this paper will be drawn from the literature on laboratory training. An attempt will be made to review thoroughly the problems of method in this area.

These problems, however, are general to research on human relations training, not specific to sensitivity training as a special method. The discussion should be of utility to those interested in conducting or interpreting research on any of the various techniques of human relations education.

In this paper I shall try to suggest a number of problems which I feel have some currency in research on human relations training. I do not wish to discourage research on training or to suggest that we postpone investigation until the ambiguities inherent in this area can be resolved by clever research designs or ingenious statistical manipulation. On the contrary, my aim is to point out some ways of avoiding difficulty as well as to indicate some difficulties which we cannot yet do much about other than simply to be aware of them. I hope that this modest catalogue may stimulate others to attack the problems to which solutions appear to be in the offing, but I hope that we shall have the courage to go ahead with the search, even in those areas where we appear doomed to live for the present with lack of rigor in design and with ambiguous, inconsistent, or even misleading findings. When all is said and done, scientific progress has never been stimulated by an unwillingness to launch investigation in the face of uncertainty or the lack of elegant tools.

I shall try, where possible, to indicate ways in which methodological barriers can be circumvented and ambiguities resolved by supplementary investigations. It is to be hoped that where I am unable to do this, others may find a way.

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1. A notable exception is the classic study by Fleishman (1951). This study showed that it is possible to construct rigorous training evaluation designs, given sufficient access to the trained subjects.

The Problem of Controls

The provision of adequate control groups for research on training is one of the most persistent methodological problems in this area. The fact that a person is in a control group biases his self-image and the perception of him by others; the fact that a person has participated in training inclines him and others to look for change in his behavior.

To compound the problem, there often is administrative or self-control over the division of a group of otherwise equivalent members of an organization into a trained group and a control group. That is, there is other than random selection as to who receives the training and when. Usually, someone makes a decision to send to training a group which is judged more likely to benefit or more willing to participate than those who are not trained. Since there is nearly always some such selection of who is to be trained, this problem is an ubiquitous one.

This problem cannot be resolved by enforced randomness of assignment to training. Sensitivity training programs are usually designed for participants who are at least nominally volunteers. If, for the purposes of research design, assignment to a training or control group is made a matter of administrative fiat, it changes the nature of the training itself. Those who have worked in nonvoluntary laboratory training settings know that the participants show a much higher degree of resistance and mistrust than is the case where attendance is strictly voluntary.

One method of administrative control which preserves a degree of voluntariness has been suggested by Massarik (1965). It involves delaying the participation of some volunteers and using them as a control group in the interim. While this method is theoretically feasible, I have found in practice that it is generally impossible to prevent at least a few persons' joining the control group who have delayed their participation in the laboratory out of ambivalence or reluctance to attend.

There are two ways around the control group problem which appear to me to be both genuinely valid and practical. Both require that we study the process of training as well as the outcomes. Instead of simply measuring participants before and after their passage through a "black box" called training, we must make some hypotheses about what it is that happens to the person which causes us to predict one outcome rather than another.

The first solution to be suggested is feasible where it is possible to give comparable groups of participants training which differs systematically along some important training process dimension. For example, the dimension might be depth of personal involvement. The variations (from right to left) might be lectures, case studies, role playing, group-oriented T Groups, person-oriented T Groups, "personal growth" laboratories. One such study by Byrd (1966) compared participants in a T-Group laboratory with those in a "nongroup" situation that used personal growth activities as the basic learning setting. Each of the groups was seen by participants and others as having undergone a significant learning experience, yet it was possible to demonstrate differential outcomes consistent with Byrd's predictions.

Another example of the use of this kind of control is a study by Bunker and Knowles (in press) in which the effects of laboratories of different lengths were compared. Bunker and Knowles also compared the kinds and amounts of change occurring for participants of different occupational backgrounds.

The assumption behind these designs is that if the amount and kind of training outcome vary systematically and predictably as functions of some input (whether the design, the type of participant, the behavior of training staff, or whatever), then the obtained changes can be viewed as "real." Because all groups being compared have been through a training experience, the design eliminates the biasing of perception which occurs when an untrained control group is used.

A variation on this theme introduces control by prediction of training outcomes through independent process variables. If we can measure some important difference in the behavior of participants or in the quality of their experience while in training, then this type of control can be achieved.

This design is illustrated by a study by Bunker in which it was found that the number of reported changes for participants in laboratory training was associated with ratings of active involvement in the T Group (Bunker, 1965). Similarly, the author found significant relationships between active involvement in training and change in categories of interpersonal perception following training (Harrison, 1966). In another study, the ratings of involvement predicted posttraining increases in consideration, as rated by organizational associates (Harrison & Oshry, 1967). This predictability of change was found even though there was no significant overall change in rated organizational behavior. If third variable prediction had not been used, the study would have resulted in a misleading negative finding rather than a finding of differential change.

Temporal Change in Training Outcome

One process which has been little investigated is the progressive change in outcome with time following training. We ordinarily give some lip service to the importance of longitudinal changes but few researchers have gathered longitudinal data. The importance of doing so is illustrated by one study the author conducted in which data were collected at two points following training. In this study of cognitive change, the changes were progressive over a surprisingly long period of time. It was predicted that changes would be greatest immediately after the laboratory and would decay with time. Change was assessed six weeks after the conclusion of training and again after six months in order to measure temporal deterioration. The results were opposite to the prediction: The changes were positive but insignificant at the six-week mark, increasing to higher and statistically significant levels only after six months (Harrison, 1966).

I believe that there is a tendency for us to study outcomes without thinking deeply about the temporal process of change. What theory there is about this process suggests that this is unwise. For example, Schein and Bennis (1965) have elaborated a three-stage temporal theory of change originally conceptualized by Lewin. According to this model, the T-Group laboratory has an initial "unfreezing" effect. The individual is "shaken up" by dissonance and discomfirmation of his self-concept through the feedback which he receives from others. The unfreezing process creates, to a greater or lesser degree, a need for change.

This is followed by a period of search and experimentation during which the individual tries out new conceptualizations, experiments with new behavior, and attempts to gather information about the effectiveness of alternate ways of relating to others. This second phase covers the period during which behavioral change actually takes place. It is followed by refreezing of behavior due both to internal forces stemming from improved adaptation and/or defense and to external forces generated by the social environment. With refreezing, the individual's behavior again comes to a quasi-stable equilibrium as close to a steady state as behavioral patterns usually come. The forces which produced the original imbalance have resulted in change up to the point where counter forces are generated within or external to the person which oppose the change and eventually restabilize behavior.

This is a reasonable though not an empirically demonstrated model of the change process. As yet no one knows how long the phases may be expected to last nor how to identify, in practice, the transition from one to another. What is clear, however, is that if this model is correct the phase a person is in makes a great deal of difference in the kind of changes we should look for as training outcomes. What we usually mean when we talk about outcomes is the "refrozen" state, in which the individual has integrated and stabilized new patterns of cognition, perception, and behavior.

What we may often measure is the process of change itself. During this phase we should expect rather different outcomes if the training has been successful. Rather than stabilized behavior patterns, we should expect to find such changes as higher activity levels and rates of interaction; greater risk taking in attempting new behavior with others; greater variability and inconsistency in behavior, values, and perceptions; and a higher level of aspiration for the quality of interpersonal relationships. These might well be accompanied by higher levels of anxiety and discomfort around the individual's self-concept and his interpersonal relationships.

It is not immediately clear what instruments or methods would best get at these qualitative aspects of behavior, but it is clear that they have not been explicitly investigated in recent studies of training outcome.

Dimensions and Directions of Change

A related problem has to do with the number of dimensions on which our design allows us to measure changes in participants. The number and kinds of degrees of freedom in our design are an implicit statement of the position we take regarding the aims and goals of training. In designing a study of outcome we express our point of view about the kinds and directions of change which we will classify as desirable outcomes. We may study only a narrow range of dimensions, in which case we are by implication narrowing the goals of training, or we may include any conceivable kind of change, in which case we are implying that one kind of change is as good as another.

The classic studies by Bunker (1965), Miles (1965), and Valiquet (in press) are examples of the latter approach. They all use a design of very wide focus. Each asked participants and their associates to give free responses describing any changes which took place in the participant's behavior during the preceding year. The obtained responses from a trained and a control group were then classified inductively, and the number of changes in each category were compared to determine the kinds of changes on which laboratory participants differed significantly from controls.

This method not only does not specify in advance the dimensions on which change is to be measured but it also tends to count a change as equally significant whether it is up or down on a given dimension. For example, it would be possible, using this method, to find significant differences between trained subjects and controls on both <u>self-control</u> and <u>spontaneity</u>, qualities of behavior which are generally considered polar opposites on a single dimension.

By contrast, a study by Harrison and Oshry (1967) of changes in organizational behavior following laboratory training used an instrument constructed deductively from Argyris' two-dimensional theory of organizational behavior (Rational-Technical Competence and Interpersonal Competence). Through factor analysis of the items derived from the theory, three dimensions were actually found and studied. Changes not measured by this instrument were, by implication, classified as irrelevant.

Furthermore, only unidirectional changes were assessed. That is, if half the participants showed increases in Rational-Technical Competence and half showed decreases of equal magnitude the net effect of the training was considered to be zero.

A Classification Scheme for Training Outcomes

These studies are examples of two of the three major categories into which research designs may be classified according to the kinds and directions of change for which the design permits assessment. Harrison and Oshry's (1967) study would be classified as <u>normative</u> with respect to outcome. It is <u>restrictive</u> with respect to the dimensions on which change was assessed and it is <u>prescriptive</u> regarding the direction of change considered desirable. Much of the writing about sensitivity training is, in fact, normative with respect to outcomes. For example, it is usually considered good for people to become more democratic, bad for them to become more authoritarian. It is good for people to be

more open to their own and others' feelings; it is bad for them to reject or suppress their emotionality.

The client's point of view is also generally normative with respect to outcomes. A personnel manager wants to know whether he can expect people he sends to a laboratory in human relations to come back more or less dominant, more or less responsive to the needs and feelings of others, more or less expressive of his own needs and feelings. He is not likely to be satisfied if he is told "it depends on the direction in which the individual needs to change."

Among practitioners there appears to be a historical trend (for example, in the laboratories conducted at Bethel, Maine, by the National Training Laboratories[2]) from a normative approach focused on the development of democratic ideology and its expression in the democratic decision-making group to a concern with individual growth as the desired outcome. The individual growth position with respect to outcome would probably focus on the receiving of feedback in the T Group as the basic learning process in laboratory training. The process begins when the individual exposes his characteristic styles of relating to others in the T Group and receives feedback about the reactions of others to his behavior. In general, we would expect that rigidly extreme styles would tend to receive negative feedback at either end of most dimensions of behavior. For example, it is common for both over-talkative and under-participating members to be pressured by the rest of the group to approach the group average in their verbal activity. Similarly, both domineering and very dependent members are likely to receive feedback which, if heeded, would move them toward a more interdependent orientation. The same moderating influence tends to be exerted on both the cold and distant and the overly warm and personal members.

Carried to its extreme, the individual growth point of view would see the T Group as the place where sharp edges are rubbed off people. A more acceptable version of this point of view would be that the T Group is a place where each individual is encouraged to explore and express the latent and underdeveloped aspects of himself. From the standpoint of experimental design, these both come to much the same thing. The Bunker, Miles, and Valiquet studies referred to above are examples of this approach, and it is noteworthy that there is not, to the author's knowledge, any study which has used this method which has failed to show significant results. The individual growth point of view seems to "fit" the laboratory training process very well.

A third position with respect to outcome has seldom if ever been represented in experimental design, but it is theoretically very important. It is represented by Bennis' 1962 article on the goals of laboratory training. According to this point of view, the objective of laboratory training is neither to teach everyone the same values and behaviors (normative model) nor to improve the adaptation of the individual by changes in values or behavior style which are tailored to his needs. Rather the objective is a general improvement in adaptive capability for all members, based on (1) improved accuracy of perception of the self and of one's relationships with others, (2) more complex and accurate cognitive mapping of the realm of interpersonal phenomena, occurring through the development of new concepts which permit the individual to comprehend a wider range of interpersonal phenomena, (3) increases in behavioral range and flexibility, through experimentation with hitherto avoided or unpracticed modes of relating to others, and (4) development of an interest in and a method for continued learning about interpersonal relationships and group phenomena or, as Bennis (1962) puts it, "learning how to learn."

Little progress has been made in measuring the attainment of such goals. Doubtless the Bunker-Miles-Valiquet method gets at these changes as well as others, but only if the describer is himself sophisticated enough to observe and report abstract similarities among behaviors which may seem quite different at the concrete level of description.

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2. On June 1, 1967, the name of the National Training Laboratories was officially changed to NTL Institute for Applied Behavioral Science, associated with the NEA.

For example, if the individual has learned to take more risks in trying new interpersonal behavior as part of his "learning how to learn," he may have exhibited this in a wide variety of ways from which the observer would have to abstract the concept, "takes more risks." Consequently, although the highest and most desirable goal of laboratory training in human relations may be this development of the individual's adaptive and learning capacity, it is doubtful that it has yet been a significant object of study, and it probably will not be until a good deal of work is put into the creation of ways of quantifying such changes. I suggest that developing these methods should have a high priority in current research efforts.

Variability in the Training Experience

There are several problems involved in the actual process of working with subjects and gathering data in research on laboratory training. A major one has to do with the difficulty of specifying the nature of the training experience which each participant has. To begin with, there exists a kind of cult of originality among laboratory trainers in which a dominant value is the invention and proliferation of new variations in training design. It thus becomes practically impossible to standardize training design except insofar as the routine inclusion of the T-Group experience may be considered standardization. A major difficulty here is that we do not yet have adequate enough theory about the effects of different elements of training design even to permit us to classify laboratories according to design. We may suspect, for example, that there are differences between laboratories in which there is a good deal of formal assistance to the participant in conceptualizing his experience through lectures, discussions, readings, and so on. We do not, however, have clear hypotheses about the relationship between having an experience, having an insight about that experience, and conceptualizing or generalizing the insight to other situations. If I had to select one aspect of training which most needs theoretical formulation and exploratory investigation, I should choose this problem of the relationship between experience, conceptualizing activities, and learning outcome.

A similar but conceptually less murky area has to do with the effects of variations in trainer style on participant learning. Though this area is far from adequately mapped, considerable exploration is going on. For example, Culbert (1966), Peters (1966), and Bolman[3] have all made recent contributions to our understanding. Hopefully, we shall soon have instruments which will permit us to assess trainer style as an independent variable and relate it to kind and extent of outcome.

Our understanding of the effects of group composition is in a similar stage of early exploration. We certainly have consistent and repeated evidence that group composition has a significant effect on the learning of participants, and we know something about the kinds of composition variables which are relevant (for a review of the literature in this area see Harrison, 1965).

It is a sobering thought indeed that if we take the effects of group composition seriously, we should try to assess the composition of each and every T Group we study. Probably, in studies using a large sample of groups, it is sufficient to randomize assignment of participants to groups and call the groups heterogeneous. It is important to remember, however, that a strong case can be made in favor of the proposition that participants learn better in groups which are intentionally composed to produce conflict between polarized subgroups (Harrison, 1965). So what we do for administrative convenience and simplicity of research design may not result in the best training of which we are capable. As an alternative, it may be worthwhile to explore the routine use of simple instruments and/or observations of participant behavior to compose groups for optimum learning.

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3. Lee Bolman of Yale University is constructing an as-yet-unpublished instrument for assessing trainer behavior along several dimensions, to be rated by participants and by the trainer himself. Personal communication. 1966.

The Timing of Data Collection

There are two problems connected with the actual administrative process of data collection which deserve consideration in any research design. One has to do with timing. After considerable experience with the administration of instruments on the opening day of a laboratory, I have come to the conclusion that the anticipatory anxiety which staff and participants alike feel during this period significantly affects the direction and variability of responses to many kinds of instruments. Unlike concerns which arise around events occurring in the process of training, this anticipatory anxiety seems largely irrelevant to any of the concerns of the research. The experience is ubiquitous but transitory, and when the training begins this anxiety is shortly replaced by more realistic concerns.

My current preference is to administer instruments by mail prior to the laboratory, if possible. If not, I sometimes wait a day into the laboratory before collecting "pre-training" data. This introduces problems of interpretation, but I have decided that I can put up with moderate ambiguity of inference in preference to gathering data at a time when most participants are upset and anxious.

Experimenter-Participant Relationships in the Laboratory Setting

Experience has also convinced me that the effects on results of the relationship between experimenter and subjects in research on training are even more significant than we are finding them to be in the academic psychological laboratory. In the latter situation, subjects are usually willing to put up with a lot of actual or suspected manipulation on the part of the experimenter in the interests of the scientific values which are shared by educated adults in our culture. In the training setting, however, we operate according to a norm which holds that the search for truth is a cooperative one which can only proceed successfully if the participants are as open with one another about their experiences, observations, and inferences as it is possible for them to be. An extremely high value is set upon the development and maintenance of trust between staff and participants and among participants themselves.

In this atmosphere, the secrecy of an experimenter toward his subjects and his real or imagined manipulation of the latter are strongly opposed by the dominant values of the laboratory training culture. If he flies in the face of these values, the experimenter runs a very real and substantial risk of being isolated and outlawed and of becoming uninfluential. Furthermore, when the researcher does place himself in this countercultural position, a good deal of the suppressed resentment which has been generated by experiences with behavioral scientists in school or at work comes to the surface. The research is thus affected not only by the relationship between the present researcher and his subjects but also by the latter's past experiences as a subject.

The net result of all this is to produce various kinds and degrees of rebellious behavior on the part of some unknown proportion of the subjects. My guess is that in most research designs this rebellion results in increased variability and unreliability rather than in systematic bias.

Both the timing problem and the experimenter-subject relationship are sources of falsely negative findings. The operation of these factors tends to produce increased error variance so that the relationships which the experimenter is seeking turn out to be in expected directions, but statistically insignificant. This may lead to nonpublication of results and/or the abandonment of a promising avenue of inquiry. I would hypothesize that the heightened sensitivity of laboratory participants to qualities of interpersonal relationships frequently combines with the experimenter's cold and standoffish style to produce such false negatives. Considerable care is required to overcome this source of error. To do so, one must usually run some danger of biased findings. This danger has been the conventional justification for the experimenter to follow his natural inclinations towards nonexposure and low involvement with subjects. I am suggesting a conscious reversal of this practice. This means being open with the subjects about one's intentions,

interests, and motives. It means making research data available wherever possible to help the participants in their attempts to learn about themselves. It means making oneself personally available to participants and showing a genuine interest in the personal growth and enhancement which they are seeking in the laboratory.

In my experience, it is possible to move in this direction a considerable way without seriously compromising the canons of experimental design. It is probably not possible to avoid any compromise at all with scientific respectability. One must make a personal choice between his estimate of the dangers of falsely negative results and his trained-in distaste for "messy" designs in which the subjects know too much. It is not possible to avoid making the choice. The respectably sanitary design runs significant risks of being a failure; the design which enlists the subjects as willing participants in the search for truth runs the risk of biasing effects. Ingenuity in design can moderate these effects, but it cannot eliminate them.

Statistical Problems in Training Research

Last, I should like to refer to some statistical problems which are endemic to research involving the measurement of change. These problems have been so well treated in a symposium on change edited by Harris (1963) that I, a nonstatistician, shall do no more than point to them. The chapters by Bereiter and Lord discuss in detail the difficulties in such procedures as measuring the relationship between initial standing on a test or variable and change on that variable, or assessing the relationship between change on a variable and an independent predictor of that change. They also explain the operation of an extremely important phenomenon in measures of change--that of regression toward the mean. Some understanding of these difficulties should be acquired before undertaking research involving measures of change so that the major pitfalls can be avoided or taken into account.

I should like to reiterate that while I believe it is prudent to be concerned about the problems of method which beset our enterprise I believe equally strongly that it is unnecessarily obsessive to be discouraged by them. We should not allow these considerations to dissuade us from conducting research. Nor should we become overly rejecting of the findings of others because they have not overcome all of the obstacles in the way of achieving certainty. The problems to which this paper has been devoted are difficulties; they are not disasters. In spite of them we have already accumulated knowledge through research which is sound enough to lead to significant improvements in practice and increases in understanding. These achievements should encourage us to learn to live with the ambiguities we cannot avoid while working to reduce those which are amenable to improved research design. It is in the service of this goal that the current review is offered.

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